



**U.S. DEPARTMENT OF COMMERCE**  
**National Oceanic and Atmospheric Administration**  
National Ocean Service  
Office of Response and Restoration  
Coastal Protection and Restoration Division  
c/o EPA Region X (ECL-117)  
1200 Sixth Avenue  
Seattle, Washington 98101

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Dear Chip and Eric:

This letter provides **NOAA's comments on EPA's June 5, 2008 draft proposal for deriving aquatic biota tissue residue values (TRVs) using a species sensitivity distribution (SSD) approach for the Portland Harbor Superfund Site.** The NOAA team involved in developing this response to EPA includes Nancy Beckvar and Rob Neely of the NOAA Office of Response and Restoration, James Meador of the NOAA Northwest Fisheries Science Center, and Bob Dexter of Ridolfi, Inc. NOAA appreciates EPA's willingness to allow an extension on the submittal of these comments in order to afford our team the opportunity to conduct a proper review. We look forward to continuing to work with EPA on developing appropriate and protective tissue TRVs for fish and invertebrates for the site.

1. One overall observation is that the document includes substantial discussion of an explanatory nature that appears to be intended to justify decisions pertaining to the TRV derivation approach. While this information may be useful for discussion purposes, we suggest that many of the points made are unnecessary and detract from the core message of the document. Also, some of these statements are potentially controversial and may produce an unintended consequence of diverting discussions and negotiations away from those issues set forward in the memorandum that are most pertinent in terms of requiring resolution.

2. NOAA is not supportive of the use of the 5<sup>th</sup> and 10<sup>th</sup> percentiles as breakpoints for the selection of TRVs from the SSDs for the protection of individual/organism-levels and community/population-levels. The focus on the Meador et al. (2002) paper as a precedent-setting application for the 10<sup>th</sup> percentile is inaccurate. This dataset also included a number of sensitive responses such as enzyme induction and alteration of thyroid hormones, because salmon undergoing smoltification are experiencing major physiological changes. These



“suborganismal” responses may very well affect their success for adapting to seawater. NOAA recommends Meador 2006, which contains a more advanced discussion on SSDs and the selection of appropriate hazard concentrations. The 5<sup>th</sup> percentile is widely accepted as exemplified by the water quality criteria (as noted in the 2006 document). We continue to suggest that the 5<sup>th</sup> percentile of the LOER SSD data is more consistent with EPA’s use of the SSD approach for community/population protection, while the protection of individuals/organisms should be based on estimates of a NOER for those species, which is best estimated by applying a factor to the 5th percentile LOER SSD.

The example justifying the use of the 10<sup>th</sup> percentile based on sediment quality guidelines is also not supported because these are not based on dose-response data for single-compound exposures. Also note the comments made by Long and Morgan (1990, p 166):

“The data assembled and reported herein were evaluated by objectively determining the lower 10<sup>th</sup> percentiles and medians in the data and by subjectively determining the overall apparent effects thresholds in the data. The same data could be evaluated using many other approaches, depending on the study objectives.... For example, the lower 5 percentile value of the data could be examined and assumed to be analogous to a level that may protect 95 percent of the species.”

3. NOAA appreciates the inclusion of a process to review the protectiveness of the TRVs for salmon and lamprey (page 11). However, we believe some discussion should be included that articulates, at least generally, how such a review would be performed.

4. The discussion on page 5 on the use of the acute-to-chronic ratio (ACR) appears to mix the type of endpoint with the exposure time. The underlying assumption is that in laboratory exposures, the expression of any endpoint, not just mortality, will occur at lower concentrations over longer (chronic) exposure times than seen in short-term (acute) studies. This observation is most likely not of concern for this effort because few sublethal endpoints are measured in acute exposures, and the study selection process prioritizes sublethal toxicity metrics..

The important point is the inherent limitation on the use of mortality values. By far, most of the values in ERED have been determined for lethality. The preponderance of these values will skew the distribution and ultimately the TRV.

Finally, NOAA still recommends that no mortality studies be included for PCBs.

5. The discussion on page 5 of the use of interspecies and field uncertainty factors also seems to mix two very different potential effects. The discussion presented appears to be reasonably applicable to lab-to field-relationships. However, the points made regarding interspecies relationships fails to recognize that species of even related taxa can have substantially different responses to the same substance. For example, lamprey are substantially more sensitive to lampricides than other fish. In addition, it should be noted that safety factors are commonly used in risk assessments (Duke and Taggart 2000).

Related to the previous point, NOAA recommends that data from plant toxicity studies not be used because the taxa differences are too great and, in many cases, plants have been found to be intolerant or stimulated by some toxicants. For many substances, the inclusion of a plant value in the SSD will raise the 5<sup>th</sup> or 10<sup>th</sup> percentile. Also note that the water quality criteria methodology outlines procedures for a separate plant value, although the water quality criteria methodology document claims that animal values are likely protective of plants. The converse of this is likely not true.

It should also be noted that an important component that was addressed in the development of the water quality criteria was time. Even though the acute values are based on short-term bioassays (48 to 96 hours), the water quality criteria only allows the acute concentration as a 1-hour average, not to be exceeded once every 3 years. Similarly, the chronic value applies to a 4-day average not to be exceeded once every 3 years. These temporal restrictions are in fact safety factors that were instituted to provide a suitable compensating period so as not to have the criterion value exceeded. For the chronic WQC, this was chosen as 4 days, which was considered substantially shorter than the 20 to 30 days used as the exposure time for most chronic tests. Based on these considerations, the tissue TRVs require the application of a safety factor to avoid true “exceedances” and provide a margin of safety.

Another related but relatively minor point is that the discussion on page 5 indicates that the selection of uncertainty factors has been “arbitrary.” In fact the choices have been subjective, based on informed best professional judgment, and therefore we suggest that “subjective” be substituted for “arbitrary.”

The discussion stating that ESA listed species (page 11) are not inherently more sensitive than other species misses the point made above that the protection of those species should be made based on NOER estimates, not LOERs. In addition, the information on relative sensitivity among the species is based on water exposures. The data required to determine whether the same relationships hold true for the concentrations in tissues do not exist or are not available.

6. The geometric means of multiple data for the same endpoints for the same species should ensure that similar dosing intervals and spacing were used, i.e. LOER from a study using only a single high dose should not be included in a mean with studies using a range of dosing intervals.

7. NOAA recommends again that tissue TRVs be developed for PAHs for invertebrates. Even if one were to accept the fact that some invertebrates have some ability to metabolize/depurate PAHs, arguably this effect would be included in the SSD data. Also, please note that the statement in the last paragraph on page 17 discussing the “rapid metabolic transformation...” of PAHs should be qualified by including “by many/most fish.”

8. NOAA believes that including tissue TRVs already accepted by EPA (page 17), is inconsistent with the process laid out in the memorandum. At a minimum, it would be helpful if the TRVs that have already been accepted by EPA were listed in the report in

association, for example, with Point 1 on page 17, along with a summary of the methods used for the derivation of these previously accepted TRVs.

9. On page 18, please provide more clarification of the process that will be used to select the appropriate statistical model for the distributions?

#### Citations

Meador JP. 2006. Rationale and procedures for using the tissue-residue approach for toxicity assessment and determination of tissue, water, and sediment quality guidelines for aquatic organisms. *Human and Ecological Risk Assessment*. 12:1018-1073.

Duke, D.L., and M. Taggart. 2000. Uncertainty Factors in Screening Ecological Risk Assessments. *Environmental Toxicology and Chemistry*, 19:1668–1680. 2000

NOAA appreciates the opportunity to provide these comments. Please let us know if you have any questions or require further clarification on any of the information we have provided via this comment letter.

Sincerely,

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